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THE AIMS OF AN ASTRONOMER.[1]

Two titles have suggested themselves for my address of this morning, 'The Aims of a Man of Science' and 'The Aims of an Astronomer.' The objections to the more restricted title are, that those of you who do not know me might think that I was about to discourse upon the inhabitants of Mars, or give you a technical paper interspersed with mathematical formulæ of appalling length. From both of these courses I solemnly promise to abstain. The broader title might lead me into domains outside of my own studies, which are always particularly tempting to a specialist. The early aims of an astronomer must be passed over briefly to reach the more alluring field when they become, or should become, the aims of astronomy.

The first aim of a boy when he reaches manhood, and becomes an independent unit in the community, is generally to acquire money or its equivalent. This aim for a time is perfectly legitimate. He is entitled to support, food, lodging and clothing. Unfortunately, the savage has here a great advantage over civilized man. As soon as he attains his full strength and physical development, he becomes an important member of his tribe. He can hunt and fish, and can live in even greater comfort than his elders. The complex wants of civilization have changed all this. With us, a boy must get his education, and for years must be dependent on others when he should

[1] Address before the Harvard Chapter of Phi Betta Kappa.

be self-supporting. Many of the evils of socialism, hatred of the rich and fear of powerful organizations, are due to this cause.

If a man never gets beyond the money-making stage, he can hardly be called a student of science. Let us assume that he is intellectually a success and attains a college position. He will never be rich, but since he is as well off as his associates he is not poor. His next aim is likely to be personal fame—a better object than wealth, but still a purely selfish one. In this stage of his development he tries to obtain honorary membership in societies, degrees or other honors, instead of waiting for them to come to him unsolicited. He makes reclamations of priority, and deposits sealed packages in the safe of the French Academy, so that if any one else should make the same discovery he can call for his package and prove that he is entitled to the entire credit, since he was first. If he is young, he attacks the work of some older man, and thus gains notoriety, even if his charges are disproved or ignored. The specious plea, 'I feel obliged, in the interests of science, to point out that my friend, Mr. A., is entirely wrong,' seldom conceals the true motive.

The next aim is higher and is for fame, not for himself but for his college, his city or his country. Enthusiasm for his state is dampened when the latter attempts to tax scientific institutions, instead of aiding them as is done in all other civilized countries. For years nearly all English mathematicians, following Newton, dealt with fluents and fluxions, while the continental mathematicians, following Leibnitz, used differential coefficients. The astronomers who gave the principal credit to Adams for the discovery of Neptune were nearly all Englishmen, while few Frenchmen admitted the claims of any one but Le Verrier.

This brings us to what should be the true aim of the student of science, the advancement of human knowledge and the determination of the laws regulating the physical universe. His sole object should be to secure the best possible results, and he must be ready to make any sacrifice of his personal wishes for this end. Astronomy thus becomes international, and wholly impersonal. To how many of us is this the one and only aim, regardless of all selfish considerations? We must not expect too much of poor human nature, and yet it can do no harm to make our ideal a high one. No man is likely to surpass his ideal, and even if it is so high that he can not hope to reach it, he may go further than if he tries only to attain money or fame. The aims of the astronomer thus become the aims of astronomy, and there is no subject to which he can better give careful attention.

No man can hope to advance science now, as has been done in the past. Think of writing a book which not only would survive and be useful for two thousand years, but which for fourteen centuries should be the great work, and practically the only authority, of its kind. Yet this is the position held by the Almagest of Ptolemy. During the greater portion of this time it was reproduced again and again by laborious hand-made copies into which errors crept, were repeated and multiplied. By far the best copy bridges more than half the interval, since it was clearly written in the uncial characters of the ninth century. It is deposited in the Bibliothèque Nationale in Paris, and in 1883 was kept in one of the show cases of that institution. It contains a catalogue of more than a thousand stars, which is perhaps that prepared by Hipparchus, nearly two centuries earlier. It not only gives the positions, but the brightness, of all of these stars, and shows that at the beginning of the Christian era the appearance of the heavens was nearly the same as at present. Even a careful observer, without instruments,

would have difficulty in detecting any differences during these two thousand years.

But for the errors in copying mentioned above, the Almagest would still give us valuable information regarding the secular changes in the stars. No worker in science knows whether his results will have any value a century hence. The work of the older astrologers was supposed, at that time, to be as valuable as that of the astronomers. No one could tell that the work of the early chemists was of more importance than that of the alchemists. Until within a century, the estimates of the light of the stars as given in the Almagest were considered as of little scientific value. One man of genius, Sir William Herschel, recognized the value of accurate determinations of stellar brightness, and from 1796 to 1799 he published four catalogues of 1,905 stars, covering two thirds of the northern sky. It was my great good fortune, when visiting his grandson in 1883, to discover the manuscript of two other catalogues, which when published rendered the work complete for the entire portion of the sky visible in England. For eighty years they had lain on the shelf, unknown to astronomers, and their existence was not even suspected. Although the observations had been made with the greatest care, the six catalogues were not in a form that could be used. The necessary reductions and publications of the results were made at the Harvard Observatory, and thus we were enabled to present to astronomers a catalogue of nearly three thousand stars, showing their brightness a century ago and determined with an accuracy which has only been equaled within the last few years.

These are examples of great successes by clear-sighted men of genius who little suspected how highly their work would be appreciated after they were dead. To offset this, there are whole generations of astronomers whose life work is now of little or no value. Let each man ask himself to which class his own work belongs. Only the future can decide with certainty, but we can at least improve methods, which will certainly do good and can do no harm.

Unfortunately, astronomical research has now become so expensive that large sums are required to carry it a step beyond what has already been accomplished. A word must, therefore, be said to men and women of wealth who desire to aid this science by gift. Many persons have learned how to accumulate great fortunes, but few have succeeded in giving away wisely large sums of money for scientific work of the highest grade. It is strange that a shrewd business man, who by life-long labor has accumulated a fortune, if he wishes to give it away, should not use the same skill that he did in acquiring it. When buying a mine he sends experts to examine it, and assures himself that he will obtain an adequate return. When converting his money into scientific results he should similarly satisfy himself that his plan is a good one, and that it will fill a real want.

Let us, therefore, hereafter have no needless duplication of observatories, no great telescopes that are idle, no costly expeditions which, owing to insufficient preparation and lack of proper organization, will surely bring no adequate return. Money placed in the hands of a suitable committee would doubtless be spent to great advantage. The Rumford Fund of the American Academy and the Elizabeth Thompson Fund are thus well and wisely administered. But it is pitiful to hear from men of the greatest ability their needs for apparatus, assistants, or means for publication, which can not be supplied by the few hundred dollars thus available.

One of the greatest needs of the physical sciences at the present time is a liberal fund for research, administered solely in

the interests of science, and by scientific men. Some of the members of such a committee should be active workers in science, some of them older investigators, still able to advise and judge, but lacking the energy of youth required to undertake research themselves. We have striking examples around us, even in this gathering, of suitable men who have passed the usual age of retirement. Some of them are still so active that they appear to accomplish even more than when they were younger. A fixed age of compulsory retirement sometimes leads to curious results. A Washington astronomer, when retired ten years ago, had all his work taken away from him and was not allowed to complete it, even at his own expense. His life is still full of work and original suggestions. An army engineer from Cambridge, too old to serve the government, has been for years, since his retirement, engaged in the greatest problems of his profession, including the Panama Canal. The thanks of Congress lengthens a man's professional career by ten years. An admiral came near having his usefulness prolonged for four years, since he was so fortunate as to be born on the twenty-ninth of February. One of the greatest and most active of living astronomers will soon be retired just as he has completed and has ready for his use the most perfect apparatus yet contrived for measuring the places of the stars. When the plan for compulsory retirement was introduced at Harvard I hoped that the observatory might profit by it. Any man can complete his own work much more economically than another. I pointed out that at the observatory we had much unfinished work, the time for my retirement was approaching, and I suggested that an appropriation should be made at once to complete it. The time is now much shorter, the work is still unfinished, and the appropriation has not yet been made.

A committee constituted as described above, and having liberal funds at their command, could advance astronomy in several different ways. My sympathy goes out to the young man who has taken a post-graduate course in astronomy, has studied abroad at a great and active observatory, and comes home to teach in a little country college. He wishes to continue his work in astronomical research with the new instruments and by the same methods he has just learned. His college has no money for such purposes, his associates do not sympathize with his wishes, and his time and strength are fully occupied with instruction. He writes a pathetic letter stating that if he had only a few hundred dollars for a certain instrument he would gladly give his own time to the proposed work. Last month I received a letter from a Jesuit priest in Buluwayo, a thousand miles from the civilization of Capetown, giving me certain definite meteorological facts resulting from a year's careful observation in that wonderful climate. He described some important observations he wished to make if he only had five hundred dollars to purchase a mounting for his telescope. The committee would not only give such a man the required aid, but also the encouragement which is often still more highly prized. The man of genius is, in many cases, sensitive, retiring, unable to promise results, or to make known his needs. He must be sought, treated with tact and encouraged. If transplanted to other surroundings, or even if supplied with better appliances, his usefulness may cease. No amount of organization would help him, in fact any interference with his plans is likely to spoil them.

On the other hand, a great observatory should be as carefully organized and administered as a great railroad. Every expenditure should be watched, every real improvement introduced, advice from ex-

perts welcomed and, if good, followed, and every care taken to secure the greatest possible output for every dollar expended. A large part of the income is used for salaries, heating, lighting and repairs. Accordingly, a small increase in the resources will produce a disproportionate increase in the scientific results obtained. Much of the work of a large observatory is routine, studying thousands of stars in the same way, the work extending, in some cases, over many years. A great saving may be effected by employing unskilled and therefore inexpensive labor, of course under careful supervision. In this way a great increase in the results can be obtained from a moderate expenditure, and the amount can be closely estimated in advance.

The clerical work is largely copying numbers on prearranged forms, and computing in which only a knowledge of the four rules of arithmetic is needed. Such work must always be checked by an experienced assistant, and all errors detected by duplicate or triplicate computations. For such routine work we pay from twenty-five to thirty cents an hour, which is much above commercial rates for similar work. Prices are much lower in Europe, and supervision would also be cheaper there. An exhibition of wood-carving and embroidery has recently been held in Berlin. Some beautiful specimens were shown which had been paid for at the rate of half a cent to three cents an hour. Less skill would be required for much of the routine work needed in an observatory. If Asiatic labor could be employed, the prices would be still less, although the cost of supervision would be greater. In India, when tiger-hunting, the beaters go into the jungle armed only with a tin pan, which they beat violently with a stick. They thus frighten the tiger and chase him towards the tree in the top of which the bold hunter is safely seated, armed with a rifle. The beaters are paid the liberal sum of three to four cents a day, which is increased to six cents if the work is done properly and the tiger is killed. The family of the beater would probably prefer that he should engage in almost any department of astronomical research. The most savage despotism of modern times was overthrown, and peace and comfort brought for the first time to the millions of inhabitants of Central Africa, by soldiers, the greater portion of whom were paid at the rate of five cents a day.

It is not unusual for the unsuccessful to criticize those who are richer and more powerful than themselves. In some countries this is done with the aid of dynamite bombs. In others (I mention no names) it takes the form of newspaper attacks on wealthy men, corporations, trusts, insurance companies and railroads. When we begrudge the hundreds of millions acquired by Standard Oil, should we not remember how much of it was earned by the genius of the men who evolved the most perfect business organization the world has ever known? If we say that Mr. Carnegie ought to distribute his millions among his workmen, let us recall the fact that he was able to sell three pounds of steel for two cents, by giving to the Bethlehem Steel Works an administration and management of every detail, superior to that of any similar corporation in existence. A great railway system may misuse a large sum of money, and yet this is a trifle compared with the thousands of millions of dollars it brings to the country by supporting a vast community of farmers who are enabled by its aid to send the products of their farms to the markets of the world. If we apply these principles to astronomy we may expect the same advance that has been accomplished in commerce, agriculture and manufacture.

Who would object to a trust whose sole

objects would be increased production, reduced cost to the public, and no profit to those forming it? The advantages of careful administration in scientific work are illustrated in a plan I detailed at the Franklin bicentenary, a few weeks ago, A telescope of the largest size entails great expense, but might produce a collection of photographs which would furnish useful material for study to half the astronomers of the world. My plan proposed that a reflecting telescope of seven feet aperture should be mounted in the best possible location, probably in South Africa, and kept at work photographing the sky throughout every clear night. An international committee of astronomers would decide to what special work the instrument should be devoted, and the photographs, or copies of them, would be distributed throughout the world to any astronomers who would make proper use of them. Copies of any or all of the photographs would be sold at cost to whoever wished for them. An astronomer of any country, prepared to undertake a particular research, would be furnished with the best photographic material that could be obtained in the present condition of science. Means would also be provided him for making suitable measurements, for reduction of the results, and finally for publication. Any competent astronomer, however isolated, would thus be enabled to carry on his researches amid his own surroundings, as well as if he were at the greatest observatory in the world. The man best qualified to discuss the results often has very little skill, even if he has the time, to take the photographs. Conditions would thus be provided which would give the best results for each portion of the work, as in any well-organized industrial enterprise. The donor would be assured that he had supplied material for study for the most expert astronomers of all countries, instead of for those at a single institution. A careful estimate of the cost of carrying out this plan showed that it would be less than half a million dollars, or about one third of that of establishing an observatory of the first class, like those now existing.

The greatest problem of all for the committee to consider, and that which would really include all the others, would be to determine which departments of astronomy were being neglected, and which were receiving attention that could better be applied to other subjects. A committee without money could accomplish little, but if a moderate sum were placed at its disposal, with the promise of more if it were well expended, astronomical science might be lifted to a new and higher plane. Suppose the subject selected were double stars. Many men of genius have done excellent work with small telescopes and poor micrometers. Such men would be supplied with the best instruments they could use to advantage, and money for recorders, computers, and publication, if they desired it.

Various systematic examinations of all stars in certain regions, and brighter than a given magnitude, have been made for the discovery of new doubles. This work should be completed for the entire sky, both north and south, according to the same system, and with similar instruments and conditions. A certain minimum number of accurate measures should be obtained of all double stars. Computers of orbits complain that many important objects are neglected, while numerous superfluous observations are made of other less interesting pairs. The committee would communicate with observers, offering aid if they would supply this want. If not, owners of large telescopes would be asked to allow them to be used for this work, the committee furnishing the necessary micrometers and employing young astronomers as observers

who would get their training, if possible, from experienced specialists in this class of work. Computers of orbits would be aided in the same way, and their work might thus be greatly improved in quality and increased in quantity. Directors of observatories could get most valuable advice and help from the committee, and when a new observatory was established its plan for work could thus be greatly improved. The Harvard Observatory would gladly welcome and profit by such advice.

The committee should not stop with existing problems. When a new line of research, like measuring the heat of the stars, is proposed, they should at once investigate it and, if the results are promising, test it. If it prove successful, they should carry it as far as present means permit. In this, as in securing the cooperation of existing observatories for any of the great problems now before us, there seems to be no limit to the results obtainable by a wise administration.

The donor, as well as the astronomer, must be asked to consider first the interests of science. His name would necessarily always be associated with his gift, and would he not prefer a world-wide, to a local, immortality? There must now be many wealthy men trying to find some good use for the money they can not take with them out of this life. The hardest problem will be to find an active committee with no taint of selfish dross. This taint exists even among astronomers. There is no more permanent, economical and efficient trustee than a great university with long continued and honorable traditions. As with any other wish of the donor, it could secure and enforce unselfish management, as well as efficiency.

Industrial enterprises half a century ago were in nearly the same condition that science is in to-day. May we not expect in astronomy the same advance by cooperation and organization? If donors, trustees and astronomers can thus be led to work for scientific results alone, regardless of country or personal considerations, it will be the best return I can make for the great privilege of addressing the Harvard Chapter of Phi Beta Kappa.

EDWARD C. PICKERING.
HARVARD COLLEGE OBSERVATORY.

SOME ASPECTS OF THE PANAMA CANAL.[1]

AFTER approximately six years of investigation, the selection of both route and type for a ship canal across the Isthmus of Panama is nearly completed. Although the report of the board of consulting engineers already made public is not final, it leads to the final consideration of the question in congress so that on the conclusion of congressional consideration work can be promptly begun under the adopted plan. Whether the final plan be that of a lock or a sea-level canal, the route will be the same, practically that of the Panama Railroad running between Colon on the Caribbean side of the isthmus and a point called La Boca on the Pacific side, a mile and a half west of the city of Panama.

The length of the Panama Canal is about 49 miles between 40-foot contours at low water at its termini, but the length between shore lines will be not more than 42 miles.

The topography of the Isthmus at the Panama crossing is well adapted to the construction of this ship canal, the original summit of the divide on the line of the canal being but about 330 feet above sea level. This has now been reduced to about 170 feet above mean sea level by the French excavation at Culebra. About one half of the length of the canal lies along low marshy ground on either side of the

[1] Read at the Ithaca, N. Y., meeting of the American Association for the Advancement of Science, June 30, 1906, before Section D, Mechanical Science and Engineering.

isthmus, making two natural sea-level sections, one about eighteen miles long on the northerly side of the isthmus, and the other about seven miles long on the southerly side; that on the northerly side running for the greater part of its length generally along the course of the Chagres River. This river has been one of the main features in the consideration of the canal work since the beginning of operations by the old Panama Canal Company in 1881. It is not a large river, as it has not more than about 800 square miles of watershed above Bohio, where in its flow toward the sea it leaves the rising ground and enters what may be termed the coastal plain, through which it meanders along a sinuous course to the ocean. It has even changed its course in the past at various locations in this marshy ground.

That portion of the canal route lying in the higher ground of the divide is but about 24 miles long, and but little more than three quarters of a mile of it had an original surface elevation exceeding 200 feet. The surface material is largely clay of ordinary character, slippery and easily moved when wet, but holding well in place when protected from the entrance of water. Below this covering of clay lies material of irregular character, as the entire isthmus is of volcanic origin. In the continental divide, at a depth varying from twenty to forty or fifty feet below the surface, an indurated clay, classed for purposes of excavation as soft rock, is found. This material gives place irregularly to hard rock at greater depths. Much of the rock of the isthmus is soft, although there is hard basalt in a number of places and, in one locality on the Panama slope of the divide, columnar basalt is found.

The work performed by the old and new Panama canal companies amounted in the aggregate to not far from eighty million cubic yards of all classes of excavation, of which possibly forty million cubic yards at most will be found available for the American construction of the canal, whether a lock plan or a sea-level plan be adopted. This work extends practically over the entire canal route, with the exception of the approach channels in the two terminal harbors, and it is nearly continuous. Over considerable stretches of the higher ground it is little more than shallow cuts through the softer surface materials, but at the great Culebra cut the material which has been excavated varies from the surface clay, readily removed by steam shovels, to hard basaltic rock, requiring blasting by high explosives before it can be removed. All the material, even the indurated clay, below the softer covering, requires blasting before it can be excavated, although the softer portions need the action of black powder only.

Except the deep cutting at Culebra, through the summit of the continental divide, the most marked work done by the old French company was the dredging through the low marshy lands from Colon to Bohio. There is at present a strip of partially completed canal about 14 miles long with a bottom width of seventy-two feet, which may be navigated by vessels drawing twelve to fourteen feet, with the exception of a short distance near Colon. Indeed, so much excavation was completed in this portion of the canal, intersecting the Chagres at a number of places, that the waters of that river have abandoned the old bed and now flow through the partially completed canal prism.

In the execution of any plan of canal one of the principal problems involved is the control of the Chagres River during seasons of flood. This problem has been considered so formidable in the past that some experienced engineers have hazarded the opinion that the Panama Canal could never be successfully completed in conse-

quence of the uncontrollable destruction that would be caused by the Chagres floods. The regimen of that river in connection with the rainfall on the isthmus has been a subject of such extensive and careful investigation that the elements entering the problem of control are now comparatively well known. Instead of the floods of the Chagres playing such a destructive part in the history of the canal, there have now been devised a number of methods of effective control, so that it can be actually demonstrated that such floods are not to be feared in any respect whatever. Indeed, if a lock plan should be adopted for construction, it would be impossible to feed the locks with sufficient water for navigation were it not for the supply offered by the Chagres River. In other words, the Chagres would play the part of a friendly agent rather than that of a vicious enemy in the maintenance and operation of the canal. While this river is subject to rapid variations of discharge, so that within a period of twenty-four or forty-eight hours it may change its character from that of a quiet, inoffensive mountain stream to a literally raging torrent, the range between low water and flood elevations is much less than in many of our American rivers. Nor are the extreme floods so formidable in character as they have sometimes been supposed. While many small floods occur every season, a high flood is a rarity, as but five have occurred within fifty years, and in no case have these high flood effects lasted more than about forty-eight hours. One of the prominent characteristics of the Chagres floods is the rapidity with which they rise, the short period of highest water and the short time required for recession to the condition before the flood began.

Records of accurate daily observations, both self-recording and otherwise, of the discharge of the Chagres under all conditions of flow have been kept for nearly twenty years, and more or less complete observations for a much longer period, so that what may be reasonably expected of the river at all seasons of the year is now fairly well known.

In addition to the discharge observations along the Chagres River, accurate rainfall records have for many years been kept for all portions of the isthmus. The amount of rainfall varies greatly both from certain portions of the year to other portions, and from one year to another. The isthmian year is divided into two parts, the wet season and the dry season. The former begins about the latter part of December and usually extends to the latter part of April, the remainder of the year constituting the dry season. During the dry season but very little rain falls; sometimes none at all for long periods. An erroneous impression may easily be received from the term wet season, which does not mean that rain falls daily, or that it often rains all day. In reality, some of the most enjoyable weather of the entire year is found in the rainy season, when there is no dust and the general temperature is agreeable. The rain falls mostly in showers, although there are continuous rain storms extending over several days. The latter, however, are not common. The average annual rainfall is not far from 130 inches at Colon and on the Caribbean side of the isthmus, but on the Pacific side at Panama the annual precipitation is only about one half as much.

These excessive rainfalls and the proximity of the two oceans produce at nearly all times an atmosphere of high humidity. The trying character of the isthmian climate is due chiefly to this feature. The temperature usually runs from 70° to 75° F. in the morning, and to 82° to 86° F. in the hottest part of the afternoon. Occasionally the temperature rises to 96° or 98°

F., but such periods of extreme heat are rare.

During the greater portion of the year the northeast trade winds blow steadily across the Caribbean Sea, so that Colon receives the benefit of the resulting winds. In fact, during the greater part of the year strong breezes are of daily occurrence in the vicinity of Colon, although these are interrupted during some portions of the rainy season.

The winds are much more gentle and far less in amount in Panama than on the northerly side of the isthmus. There are periods of gentle breezes from the north and also from southerly directions, but they are never high winds. The Bay of Panama, on which is located the city, and into which the canal will lead from the north, is so free from high winds that shipping at anchor in it never needs the protection of breakwaters or similar structures. A real wind storm of high intensity in that vicinity is practically unknown. This condition is so strongly characteristic of the Bay of Panama that many of the opponents of the Panama route have strongly argued against it for the reason that the prolonged calms and general absence of winds would make it difficult for sailing vessels either to approach that end of the canal or to leave it after having passed through the waterway.

The harbor of Colon, completely open to the north, is of a radically different character. While it is frequently visited by breezes and winds of ordinary intensity, there are not, on the average, more than three or four days at most in the year when winds are high enough to be troublesome to shipping lying there. During those three or four exceptional days, however, wind storms of great violence, called 'northers,' may blow. At such times no ship can safely lie at anchor in Limon Bay, on which Colon is located, nor can they lie berthed alongside of the piers. In both cases they are in grave danger of being wrecked. It is the universal custom during the period of 'northers'—November, December and January—for every ship fitted with power quickly to leave the Bay of Limon and either put to sea while the storm lasts or seek the small naturally protected harbor of Porto Bello, about eighteen miles to the northeast from Colon. This characteristic of the harbor of Colon will make necessary the construction of great breakwaters or other similar works, in order to transform it into a suitable terminal harbor for the Panama Canal.

The report of the board of consulting engineers discloses a radical divergence in the views of its members. A majority of eight of thirteen members, including the five foreign members, have reported unqualifiedly in favor of a sea-level canal, having a bottom width not less than 150 feet in ordinary section, a minimum depth of water of 40 feet, and a top width in ordinary section of 270 feet except for a distance of seven miles in the great Culebra Cut, where the canal prism would be in rock with a width of 200 feet. All sides in rock excavation would be vertical. Inasmuch as the maximum range of tide in the Bay of Colon is never more than about two feet no tidal lock would be required at the Caribbean entrance of the canal. In the Bay of Panama the maximum range of tide may reach nearly twenty-one feet. The board, therefore, assumed that a tidal lock would be required, or at least should be planned and estimated for at or near the Panama end of the canal. This lock is to be located near the easterly side of Sosa Hill, not more than one half mile from the shore of Panama Bay. It is designed to have a usable length of 1,000 feet and a usable width of 100 feet.

The control of the floods of the Chagres River would be effected in the majority

plan by a dam across the Chagres River at Gamboa, about thirty-one miles from Colon, near the point where the Chagres River in its downward course first cuts the canal line. At that point bed rock is about fifty-five feet below the surface of the water in the river, affording a comparatively easy masonry construction resting directly upon bed rock, and thus securing an undoubted foundation for a dam. The maximum elevation of water surface in this lake would be about 120 feet above the present surface of the water in the river at Gamboa, or about 170 feet above mean tide. The dam would be fitted with suitable controlling gates of sufficient capacity to meet the requirements of the highest floods. The available storage volume created by this lake would be sufficient to take in quick succession two of the greatest floods which have ever occurred in the Chagres River, so far as is known either by exact records or by reliable report.

It would be the purpose in this system of control to allow flood waters of the Chagres River to escape through the controlling gates into the canal prism at a maximum rate not exceeding 15,000 cubic feet per second, producing a current in the canal, if all flow should be in one direction only, of about one and one quarter miles per hour, a negligible quantity as far as its effects on navigation are concerned. It would require but two or three days after high floods in the Chagres to depress the surface of water in the lake so as to be in readiness for another flood whenever it might occur.

The only other stream of magnitude, discharging into the Chagres River within limits affecting the canal, is the Gatun River, which joins the Chagres near the little native town of Gatun, seven miles from Colon. The discharge of this river, however, would be carried into Manzanillo Bay entirely outside of the canal in an independent artificial channel on which much work was done by the old Panama Canal Company. The other and much smaller streams intersecting the canal line throughout its entire course would be either kept out of the canal altogether by dams high enough to reverse their flow into other drainage basins than their own, or received into settling basins outside of the canal and quietly discharge their small flows into the canal prism over wiers in the usual manner. All streams of this latter class, however, are extremely small. By these means all sensible amounts of silt or other heavy material carried in floods would practically be kept out of the canal, thus reducing the cost of maintenance in this respect to a small annual amount.

A minority of five of the consulting board reported their judgment in favor of a lock canal with two terminal lakes and with a summit level eighty-five feet above the mean level of the ocean. In this plan it is proposed to construct a great earth dam 135 feet high across the Chagres River at Gatun. This dam would retain a large lake backing the water in the river up to Alhajuela, a point over thirty miles from the site of the dam. The surface of the water in this lake constitutes the summit level of the lock plan. Three locks in series or flight, each with a clear length of 900 feet and a clear width of 95 feet, would be built at the site of the dam, each with a lift between 28 and 29 feet to pass vessels up from the approach channel leading to the locks from the harbor of Colon to the summit level. The southern extremity of the summit level of this plan would be at Pedro Miguel on the southerly side of the continental divide where the channel issues from the Culebra cut. At this point, between thirty-nine and forty miles from Colon, there would be located a lock with a lift of about thirty feet, connecting with the terminal lake at the

Panama end of the canal, which would have a water level of about fifty-five feet above mean tide in Panama Bay. This terminal lake would extend to another earth dam, or rather two earth dams in the vicinity of the present railroad terminus at La Boca and one embankment easterly of this location. That portion of the dam at La Boca would be built directly across the mouth of the Rio Grande River, which is chiefly a tidal estuary. A hill known as Sosa Hill would separate the two earth dams already alluded to, and in this hill would be two locks in series connecting the terminal lakes with tide water in Panama Bay. All the locks required by this plan would be built on the twin system, so that one set would always be in use if the other should be disabled or be out of use for repairs. All ships passing across the isthmus in such a canal would have to be lifted up to an elevation of 85 feet at one end of the canal and dropped down the same amount at the other.

The control of the Chagres River in the lock plan would be effected by the lake formed by the dam at Gatun. Its volume would be so large that the waters of the highest floods could be received into it and discharged through suitable controlling gates constructed in the dam without varying the elevation of the lake more than two or three feet at most. Also, the lake would act as a reservoir for the water required to feed the locks during their operation.

There are at present being built ocean steamers for the traffic between Liverpool and New York about eight hundred feet long. This fact coupled with the rapid increase in ocean steamship dimensions during the past fifteen or twenty years caused the board by an almost unanimous vote to record its judgment that locks proposed for the isthmian canal should have a usable length of not less than one thousand feet and a usable width of not less than one hundred feet, but the minority abandoned this position in their plan and recommended the smaller locks which have already been mentioned.

The total estimated cost of the sea-level canal is about $247,000,000, including a twenty-per-cent. allowance for contingencies, administration and engineering. The estimated cost of the lock plan with the reduced size of locks is about $140,000,000, including the same twenty-per-cent. allowance found in the above estimate of cost of the sea-level plan.

In the recent examination before the Senate Committee on Interoceanic Canals, however, it was brought out that the minority allowance for the cost of lands submerged by the terminal lakes of its plan was entirely inadequate. It was shown that on the basis of prices already paid by the United States government, and by the new Panama Canal Company for somewhat similar lands, and in view of the claims already made by alleged owners, this amount of land damage might reach an additional sum of $15,000,000 to $18,000,000, and that if the excess of annual cost of maintenance and operation of the lock plan over the sea-level plan be capitalized at prevailing rates of government interest the real cost of the lock plan might approximate not less than $175,000,000 to $180,000,000.

The question of the selection of ultimate type of canal to be adopted for construction has been submitted by the President to Congress, and the Senate Committee on Interoceanic Canals has been conducting an extended examination of the entire question. That committee has called for evidence in detail from the advocates of both types of canals, but its report has not yet been made.[2]

The advocates of the sea-level plan con-

[2] Since preparing the MSS. for this article the Senate Committee has reported in favor of the sea-level plan, but Congress has adopted the lock canal.

tend that the only suitable canal across the Isthmus of Panama, commensurate with the great interests involved, especially those of the United States, is one which will permit the safest and freest passage of traffic. It is contended that there should be practically no obstruction to a free transit across the isthmus; that any canal now constructed should accommodate conveniently and freely the greatest ships afloat, and be of such a character as to admit of the easiest and most economical enlargement in the future if it should ever be required, and that the sea-level plan only fulfills all these fundamental requisites. Although there would be a tidal lock at the Panama end of the canal, inasmuch as the range of neap tides may not exceed seven or eight feet, the gates of that lock would be wide open at least one half of the time. There are hydraulic engineers of repute, who even believe that this lock would not be necessary. The majority of the board further contends that the operation of three locks in series would be a source of grave danger, indeed, a continual menace to the safety of the large ships passing them. Serious accidents caused by ships ramming the gates of both the Manchester Canal in England and at Sault Ste. Marie, Mich., show that it would be possible, and even probable, that some ship approaching the upper of these gates might ram them out of position and plunge down through the entire series.

Although the minority claim an annual capacity of sixty to seventy million tons of traffic for its lock plan, this claim has been strongly contested, and evidence brought out before the senate committee indicated that it would be more reasonable to place the maximum annual capacity at one half that amount or less.

Again the safety or stability of the earth dams, on which the very existence of the minority plan is based, has been most seriously questioned. A considerable number of borings made into the material on which it is proposed to place the Gatun dam show that the maximum depth of that material is not less than 258 feet; that it is largely sandy, and in some places gravelly and freely water bearing, *i. e.*, permeable at various depths from thirty-two feet below the surface down to nearly two hundred and fifty. These conditions, it is alleged, might lead to dangerous percolation under the dam, and so bring its stability into grave question. Somewhat similar criticisms have been made regarding the earth dam across the tidal estuary of the Rio Grande.

Although the capacity of the sea-level canal has been also criticized or questioned, that capacity was shown to be practically unlimited. Although most serious congestions of ships arriving in groups or fleets at either end of the lock canal might lead to long delays in some cases with a lock canal, it was shown that no such congestion could occur with the sea-level canal, even at the Panama end, for the half of any tidal period would permit any group of ships to pass into the canal while the gates were open.

The one hundred and fifty feet bottom width of the sea-level plan exceeds the width of even the turning out or passing places in the Suez Canal. In other words, the Panama sea-level canal would be a continuous passing place throughout its entire line. The total curvature of the sea-level plan was also shown to be less than that of the lock plan, and but little different from the total curvature of the Suez Canal. The lock plan with the large terminal lakes has the further serious disadvantage of possessing a minimum adaptability to transformation to a sea-level canal in the future. While this transformation is a possibility, the great difficulties attending it, and the excessive cost of that trans-

formation, were considered by the board as a whole to make transformation of this plan essentially not feasible.

Again, inasmuch as the canal would probably not be completed and opened within less than ten years on any plan the locks of the minority plan would not be large enough to accommodate ships then afloat if the rate of increase of ships' dimensions during the past ten years should be nearly reached during the next ten.

The time estimated by the majority to be required for the building of the sea-level canal is from twelve to thirteen years after making the most abundant allowances for the effects of climate, of the rainy seasons, of the necessary repairs and renewals of plant, of the eight-hour labor day, of the low efficiency of available labor, and without working more than one shift of labor within twenty-four hours. It is believed that the investigations of the majority show, however, that there is a reasonable probability of a sea-level canal being opened in from one to two years less time than their estimate.

The time estimated by the minority as necessary for the construction of their lock plan was ten to eleven years. As the construction of this plan involves a much higher grade of labor, and a far larger amount of so-called works of art, such as the locks, involving the making and putting in place of about 3,500,000 cubic yards of concrete, than the sea-level plan, the writer believes that a lock plan with a summit level eighty-five feet above mean tide can be executed in little if any less time than a sea-level plan.

The recent dreadful earthquake disaster at San Francisco constitutes the gravest warning in human experience of the advisability of constructing this canal in such a way as to give it the greatest degree of immunity from the results of any convulsion of nature. The isthmus of Panama is a region of rather frequent earthquakes, but they are not often severe. It would be an act of folly, however, to ignore the lesson of such an appalling catastrophe. The canal which is to be constructed across the Isthmus of Panama should be of such a type as to give the minimum of obstruction, either natural or artificial, the greatest degree of safety not only in operation, but from the effects of earthquakes, against the severity of which there is absolutely no insurance whatever, and the sea-level is the only type which fulfills these imperative requisites.

Wm. H. Burr.

SCIENTIFIC BOOKS.

Archeological Researches in Costa Rica. By C. V. Hartman. Stockholm, Ivar Haeggströms Boktryckeri, A.B., 1901. 4°, 195 pp., 488 text illus., 1 map and LXXXVII pls.

Museum collections and special publications derive their value from the character of the field-work on which they are based. It is with such material as Mr. Hartman has furnished and by means of the methods he employed that we may hope to raise American archeology to the dignity of a real science.

In the growth or decay of art, industry, customs, religion, there must of necessity enter the time element. For this reason, systems of relative chronology play a most important part in prehistoric archeology. A careful, intelligent, thorough study, therefore, of the contents of graves is absolutely indispensable. 'Archeological Researches in Costa Rica' is by no means confined to a study of burial places, yet it describes fully more than 400 graves.

Mr. Hartman's field investigations were carried on during the years 1896–97. He began his researches on the east coast with the great mound and walled enclosure at Mercedes. The mound is about 300 meters west of Rio Novillo; is truncated, with diameters at base and top of 30 and 20 meters, respectively. The height, 6.5 m., is the same as that of the surrounding wall. The purpose of the mound 'seems to have been to serve as a platform, or temple, for the large statues, which were

placed with faces towards the rising sun.' The author suggests that a wooden structure with thatched roof may have covered the mound, while the 'flat stone walls apparently only served for enclosures.' Four human figures and one of an alligator were found at the base of the large mound. A small mound at the end of the northern stone wall was also investigated, but neither it nor the large mound was completely explored.

In the thick forest near the enclosure there were graves that had previously been opened. They were all of an oval or rectangular shape. Only after careful search in the 'thorny thicket, beneath the gigantic forest trees' were hitherto undisturbed graves discovered. "They were arranged in small groups inside a common and almost circular platform, or enclosure, usually about half a meter in height." Each enclosure was bounded by a margin of stones of various sizes. The groups, each containing from three to five graves, are described. As regards construction the graves at Mercedes all belong to one class. The walls are still in perfect condition except when interfered with by the roots of trees. Partially chipped limestone slabs form roof and floor.

Only one grave contained traces of bone. In most of them pottery was found. "The majority of these vessels show signs of wear and are covered with soot proving that they were employed as cooking utensils." Other objects found were: Charred corn and corncobs, two stone celts and a bead of blue glass. This latter being of European manufacture, gives a clue to the age of the burials (not earlier than the sixteenth century). In these regions it has been the custom to bury the dead inside the houses, so that in all probability the low platform enclosures, containing the graves described, were primarily foundations for the huts of the living.

A curious discovery was made about 100 m. from the temple mound, where the 'rains had swept away the humus and uncovered a small portion of the roof of a stone chamber, containing a lot of pottery, but no proof that it had been used as a grave.' The construction of the chamber, which differed in several respects from the graves and the amount of pottery contained therein, leads the author to look upon it as a cache.

Not the least interesting find made in the neighborhood of Mercedes was a stone-cutter's workshop. It was discovered by chance while opening forest paths in search of graves. "Over a surface of some 20 square meters and to a depth of about one meter, the soil was intermingled with chips and partly finished idols."[1]

Rains making work on the Atlantic lowlands practically impossible the highland valleys of the interior were chosen as a field of exploration, but not before investigating a number of sites known to the occupants of the neighboring haciendas. These included a stone enclosure at Williamsburg and graves at Siquirres, Sta. Rosa, Guasimo and Jiminez.

As to work in the highlands, the valley of Cartago was selected as the most promising. Near Santiago, twelve stone cists and as many grave-pits were found in a flat elliptical mound, the cists being arranged in the eastern half and the pits in the western half. The latter differ in construction from the cists, being bowl-shaped and formed of cobblestones. The cists were all quadrangular, with 'roof, sides and bottom composed of thin slabs of limestone.' In both forms of grave, a number of objects, chiefly pottery, were found.

There were no traces of bones. The graves were not long enough to admit of adult burial at full length. It is presumed that these graves were used as repositories of the bones only after removal by decomposition or otherwise of the soft parts, a presumption strengthened by the fact that 'quite small stone cists were found in several places to contain remnants of skeletons, which had been apparently deposited in them in bundles.'

The vessels in the Santiago stone cists were, as a rule, well preserved; few bore marks of use. In fact only a few are large enough to admit of practical use. They are probably symbols of larger vessels. Later researches on

[1] It was for his work on the east coast that the Loubat prize was awarded to Mr. Hartman by the Swedish Academy of Literature, History and Antiquities.

the Pacific coast brought to light a quantity of diminutive mortuary vessels.

At Chircot, a suburb of Cartago, the ancient capital of Costa Rica, was found 'a really extensive ancient burial ground, which had been left almost undisturbed.' The superficial part only of the enclosing stone-circle had been removed by cultivators of the soil. Here were 'crowded together in a small oblong space' 20 meters long by 15 in breadth, 205 graves. They were arranged in three layers. The cists of the lower, or oldest, layer were in three groups; those of the middle layer, in four groups; while the cists of the top layer were fairly evenly distributed over the whole burial ground.

The Chircot cists were in shape like those at Santiago and many were diminutive in size. In most of the cists that were of sufficient length, the 'skeletons lay stretched out on their backs at full length.' In the small cists, the bones, when present, were in heaps or bundles and often did not represent the entire skeleton. The skulls were dolichocephalic.

The mortuary vessels were not always in the cists. Many were found just outside, usually at the head end and, in a few cases, on the roof. Of the relics deposited inside the cists, the majority were in the vicinity of the head and trunk; a few were near the feet.

Objects in stone were rare. Of large, crude vessels intended for household purposes, none were found; nor any object of European origin. While the burial place must have been in use a considerable length of time, there were no cultural differences between the graves and contents of the lowest layer of burials and those of the highest. A majority of the cists contained no relics at all. In each of the others from one to five were found.

About 50 meters east of the first burying-ground a second, somewhat smaller, was found. It had been, in part, demolished. The cists that remained were similar to those of the first field and arranged in two or three layers. In the small cists were 'fragments of skeletons deposited in bundles, usually one in each cist; but in some, two or three.'

At Los Limones, 6 kilometers from Cartago, two elliptical mounds were found. They were not more than a meter high at the center, and the stone margins, if they ever existed, had disappeared.

There were 26 cists in the first mound and 39 in the second. The graves were of the same construction as those at Chircot, and were, for the most part, without artifacts. Stone objects were especially rare.

Orosi, the next site investigated, is in the mountains some 10 kilometers southeast of Cartago. It is an ancient dwelling-place selected because of a desirable water supply and adjacent stone quarries. The general ground-plan shows a number of circles and semicircles, bordered by stones and varying in size from 8 or 10 to 20 meters in diameter. In the midst of these is a rectangular space 27 m. long by 17 broad, surrounded by low embankments—probably an open courtyard.

Five of the stone enclosures containing graves were excavated by Mr. Hartman. It was here he found an obsidian flake—'the only object of obsidian met with during all the excavations in the highlands.' Obsidian is also rare on the Pacific side, the author finding 'not even a single chip of obsidian' during his excavations there. One of the stone circles at Orosi was the richest in relics of any of the highland mounds.

About 1,000 stone objects were collected from a neighboring coffee plantation, where a number of partially demolished stone circles were still visible. In one of the cists opened, a golden bell was found; in another, two small tubes of rolled copper. Here stood, also, an upright stone slab with petroglyphs.

The volume represents the results of a year's scientific investigations carried on first along the Atlantic lowlands and later among the highlands of the interior. The culture is everywhere "that of a Stone Age people of high standing, possessed of ornaments of gold and copper, but with no tools or weapons of metal at all." No date can be fixed for the beginnings of this culture, but that it continued to exist after the arrival of the Spaniards is attested by the presence of glass beads in some of the graves.

Of native artifacts, only two implements of bone were found. The remainder were of

clay, stone or metal (gold and copper ornaments only). The great majority of the clay vessels were evidently intended for mortuary purposes only. They testify to a 'highly elaborated technique and cultivated taste,' but do not include any types that come up to the best there is in Chiriqui ceramics.

Two types of ornamentation are particularly noticeable: (1) Incised geometric designs; and (2) punctate knobs resembling raised tattoo marks, or scarifications. The author observes 'that certain classes of ornament seem to have been allotted to certain classes of vessels.'

The ancient Guëtares of Costa Rica seem to have excelled in the manufacture of large multicolored bowls, a number of which have been reproduced in color, thus adding attractiveness to what even without them would be a superb series of plates.

This large quarto volume is published at the sole expense of Mr. Åke Sjögren, who has also given the collection on a part of which the work is based, to the Royal Ethnological Museum in Stockholm.

GEORGE GRANT MACCURDY.
YALE UNIVERSITY MUSEUM,
NEW HAVEN, CONN.

SCIENTIFIC JOURNALS AND ARTICLES.

The Botanical Gazette for June contains the following papers: K. M. Wiegand publishes an account of his researches on the conditions of buds and twigs in winter, his observations leading to many conclusions entirely in variance with accepted notions. S. Yamanouchi publishes a preliminary account of his investigation of the cytology of *Polysiphonia violacea,* showing definitely the alternation of generations. H. F. Weiss describes in detail the structure and development of the bark in sassafras. E. J. Hill gives an account of the distribution and habits of the common oaks of the Lake region.

WE learn from *The Botanical Gazette* that a new journal, bearing the title *Annales de Biologie Lacustre,* is to be published under the editorship of Dr. Ernest Rousseau, with the cooperation of a large board of editors. The first fascicle is announced to contain 192 pages, with figures and maps. Publication is to be in German, English, French and Italian. Each volume will contain 400 to 500 pages, and the subscription price will be twenty to thirty francs. The address of the editor is Musée Royal d'Histoire Naturelle, rue Vautier, 31, Brussels.

Science Progress, published from 1894 to 1898 under the editorship of Professor Bretland Farmer and the general direction of Sir Henry Burdett, has been revived under the name *Science Progress in the Twentieth Century.* The editors are Dr. N. H. Alcock, lecturer on physiology at St. Mary's Hospital Medical School, and Mr. W. G. Freeman, F.R.S. The journal is published quarterly by Mr. John Murray. The contents of the first number are as follows: 'A Science of Commerce and some Prolegomena,' by W. J. Ashley; 'Chloroform a Poison,' by B. J. Collingwood, 'Physical Geography as an Educational Subject,' by J. E. Marr; 'On the Occurrence of Prussic Acid and its Derivatives in Plants,' by T. A. Henry; 'The Solvent Action of Roots upon the Soil Particles,' by A. D. Hall; 'Some Notable Instances of the Distribution of Injurious Insects by Artificial Means,' by Fred. V. Theobald; 'The Blood-Platelets,' by G. A. Buckmaster; 'Some Recent Progress in Chemical and Structural Crystallography,' by A. E. H. Tutton; 'The Geological Plans of some Australian Mining Fields,' by J. W. Gregory; 'The Corn Smuts and their Propagation,' by T. Johnson; 'Nehemiah Grew and the Study of Plant Anatomy,' by Agnes Robertson; and 'The Utilization of Proteids in the Animal,' by F. G. Hopkins.

DISCUSSION AND CORRESPONDENCE.

DE VRIES AND HIS CRITICS.

THE followers of Darwin in the early sixties had two difficult tasks to accomplish. One was to induce people to give the theory of natural selection an unbiased consideration; the other, and more difficult one, was to get them to understand it.

"I have often found the most extraordinary difficulty," wrote Darwin to Carpenter,[1] "in making able men understand at what I was

[1] 'Life and Letters,' Vol. II., p. 18.

driving." And as the display of mental density increased with time, he wrote to Hooker,[2] "I am inclined to give up the attempt as hopeless. Those who do not understand, it seems, can not be made to understand."

The scientific world has largely learned the first lesson—that of open-mindedness—but two recent articles in SCIENCE[3] suggest that we have not learned the other.

It is not my intention here to pose as an advocate of de Vries, nor as a self-appointed interpreter of the mutation theory. The writings of its author are too clear to need any additional elucidation. But it is important, before we discuss the theory, to be sure that we thoroughly understand it. A perusal of the articles just cited makes it difficult to believe that their authors have given the writings of de Vries a very thoughtful reading.

Calling attention at the outset to the importance of understanding the terms used by de Vries, the author of the first article says: "What we systematists have been in the habit of calling spontaneous variations or '*sports*' he calls '*mutations.*'" This is doubtless one of the commonest, and at the same time most fatal, errors to a correct understanding of mutation. 'Mutations' and 'sports' are by no means synonymous terms, as used by de Vries, and, after he has devoted several pages to carefully defining these terms, and to showing that, while mutations are a kind of 'sport,' *not all sports are mutations,* it seems difficult to understand how one who had gotten his notions of the theory at first hand could persist in the former loose usage of the terms. The distinction drawn by de Vries is clear. For example, he says,[4] "in order to avoid confusion as far as possible, with the least change in existing terminology, I shall use the term 'ever-sporting varieties' for such forms as are regularly propagated by seed, and of pure and not hybrid origin, but which sport in nearly every generation." Citing the striped variety of the larkspur as the first illustration, he continues: "Such deviations are usually called sports. But they occur yearly and regularly,"[5] and for this and other reasons they are not mutations.[6] Of these later.

Again our critic tells us that, "What we call *individual variations* he calls '*fluctuations.*'" And yet on page 77 of 'Species and Varieties' we read, "From a broad point of view, fluctuating variability falls under two heads. They obey quite the same laws and are, therefore, easily confused, but with respect to questions of heredity they should be carefully separated. They are designated by the terms individual and partial fluctuation." Individual variation and fluctuation, then, are not synonyms, for some fluctuations are 'partial' and not 'individual,' and this difference is explained and illustrated in the following twenty-three pages.[7]

On page 242[8] we are told that de Vries "appears to have been carried away with enthusiasm over his discovery and jumps to the conclusion that species in general originate by mutation—*and in no other way!*"[9]

'Species and Varieties' does not profess[10] to treat at length the problem of hybridization, yet lecture IX. deals with the subject, and on page 266 the author refers to the fact that "Kerner von Marilaun pointed out the fact long ago that many so-called species, of rare occurrence, may be considered to have originated by a cross," and nearly a score of well-authenticated illustrative examples follow, with the sanction of de Vries as to the hybrid origin of the species.

"And has any reason been brought forward to justify—much less necessitate—a change in this belief?"[11] (*i. e.,* that differences in char-

[2] *L. c.*, Vol. II., p. 109.

[3] Merriam, C. Hart, 'Is Mutation a Factor in the Evolution of the Higher Vertebrates?' SCIENCE, February 16, 1906. Ortmann, A. E., 'The Fallacy of the Mutation Theory,' SCIENCE, May 11, 1906.

[4] 'Species and Varieties,' p. 310.

[5] *L. c.*, p. 11.

[6] Mutations are distinguished from other kinds of sports, among other ways, by being 'of very rare occurrence.' *L. c.*, p. 191.

[7] See also pp. 190 and 191, and lecture XXVIII.

[8] Merriam, *l. c.*

[9] Italics mine.

[10] Cf. p. 250.

[11] Merriam, p. 247.

acter can be explained by the slow and gradual accumulation of individual variations). The whole book, 'Species and Varieties,' and the two German volumes of 'Die Mutationstheorie' at once furnish, to one who understands them aright, more than sufficient reason that the old theory must, to say the least, stand the test of the most rigid re-examination, and the crucial test of verification by experiment.

And what, pray, has 'the practically unanimous *belief* of zoologists and botanists the world over' to do with the merits of a new theory? Did they not practically all, previous to Lamarck, believe in special creation? "The old saying, *Vox populi, vox Dei*," wrote Darwin,[12] "as every philosopher knows, can not be trusted in science."

In the same paragraph, this: '* * * a species appears [*sic*] to have arisen in a slightly different way,' etc. (Of course it is an elementary species for which such a claim is made.) Why question the veracity of the author of the mutation theory by using the word 'appears'? When, since the publication of Darwin's 'Origin,' has a scientific fact been supported by a greater amount of experimentation, more carefully and more fully recorded, and not by one man only, but by several workers?

And if the critic asks, "How does it follow that 'then in truth Darwinism can afford to lose the individual variations as a basis'?"[13] the answer is: because the bottom does not fall out of the theory by such a loss. Other material remains for natural selection to work on. 'If it can be proved that a man eats beefsteak for breakfast'[14] it does not follow that, 'of course he could not have eaten bread.' But, if it were demonstrated by reiterated experiments that bread can do for a man all that beefsteak does, then the man 'can afford' to do without the steak. Or, to make a more relevant application, some men may breakfast on steak, and others secure the same ends with bread alone.

Referring to his extended field studies of plants, the critic says, "These studies have convinced *me* that with plants, as with animals, the usual way in which new forms (subspecies and species) are produced is by the gradual progressive development of minute variations."[15] Several pages of 'Species and Varieties'[16] are devoted to examples illustrating that 'long-continued selection, alone, has absolutely no appreciable effect' in changing the inner nature of a species or of a race, whereas there is experimental evidence of another factor by means of which such a change is accomplished. The beauty of experiment is that it convinces *all*, because given results may be produced by all alike at will, and 'experiments are a repetition of things occurring in nature.'[17]

When one says he has 'passed in review more than a thousand species,' we are constrained to ask, 'which, of at least three or more commonly recognized kinds of species?' Obviously, for example, the intergrading species of a genus like *Cratægus* (hawthorn), would prove nothing germane to the subject. And if the best result of this laborious examination of species is, 'without finding a single one which *appears* to have originated in this way,'[18] one recalls the fascinating account in 'Species and Varieties'[18] of 'the first experimental mutation of a normal into a peloric race.'[19] "The step from the ordinary toadflax to the peloric form is short, and it *appears* [note the word] as if it might be produced by slow conversion."[20] What a fine pseudo series one might arrange with the normal *Linaria vulgaris* and the *L. vulgaris peloria* at the extremes, connected by the ordinary fluctuating peloric variations![21] The beauty of de Vries's method is that it is possible 'to arrange things so as to be present when nature produces * * * these rare changes.'[22] In this way it is *known* (not a matter of opinion)

[12] 'Origin,' 6th Am. ed., p. 143, 1883.
[13] Merriam, *l. c.*, p. 243.
[14] Merriam, *l. c.*, p. 243.
[15] Merriam, *l. c.*, p. 243.
[16] P. 790 *et seq.*
[17] 'Species and Varieties,' p. 430.
[18] P. 464.
[19] 'Species and Varieties,' p. 473.
[20] 'Species and Varieties,' p. 465. Italics mine.
[21] Cf. also 'Species and Varieties,' p. 249.
[22] 'Specics and Varieties,' p. 465.

that "The mutation took place at once. * * * No intermediate steps were observed. * * * Not a single flower on the mutated plant reverted to the previous type."

"According to *degree* of development at the time of their first appearance they [variations] may be designated *ordinary individual variations*, or *sport variations*. This is an old story."[23] Exactly! The new story is de Vries's very careful distinction between different kinds of sports.

And when one reads that "his [de Vries's] studies of plants have been mainly with species as modified by man rather than with species in a state of nature,"[24] one can not help wondering if lectures II., XII., XVI., XX. and XXIII. have been read at all.

And again when it is stated that "As a matter of fact, subspecies in nature do not occupy the same ground with the parent form, but an adjacent area," one can only wonder why a statement that may be quite true of a given group of animals (*e. g.*, birds) is made so general as to include the entire plant kingdom. In what sense, we may ask, is the term subspecies used? If by it is meant geographic variety,[25] then, of course, groups that do not occupy the same region do not occupy the same region. But the term is not used in this sense in systematic botany.

And if the mutation theory seems to any one 'burdened' 'with the additional requirement that in giving off new forms the old is not altered,' relief will come to him by indulging in a little pedigree-culture experimentation, and then digesting what 'Species and Varieties' has to say about the destruction of the unfit and the survival of the fittest (elementary) species in 'intra-specific selection.'[26]

Can it be possible that, after reading 'Species and Varieties' and the larger German work, the only source of an adequate explanation of interspecific gaps is unlimited and favorable field experience, and unlimited experience in handling specimens, etc.?[27]

"Inasmuch as sudden or sport variations are exceedingly rare[28] while slight variations are exceedingly common, does it not follow that the vast majority of species must originate from slight variations?"[29] Possibly, if it can be shown that the origination of new species is an exceedingly common affair, and a matter of frequent observation. So far as the records show, no one, up to the time of de Vries, had ever knowingly observed the origin of a new species or natural variety.

And when we read that "One might spend a lifetime in studying animals and plants in the interior of almost any of the faunal [and 'floral,' we suppose is to be understood] areas without encountering transitional forms or intergrades,"[30] it almost seems as though we were reading de Vries. The mutation theory not only offers a possible explanation of this fact, but would even lead one to expect such a condition.

Some ten years before the publication of 'Species and Varieties' Bateson inquired,[31] "Is it not then possible that the discontinuity of species may be a consequence and expression of the discontinuity of variation?" and then added:

> Upon the received hypothesis it is supposed that variation is continuous and that the discontinuity of species results from the operation of selection. For reasons given above (pp. 15 and 16) there is an almost fatal objection in the way of this belief, and it can not be supposed that all variation is continuous and also that the discontinuity of species is the result of selection. With evidence of the discontinuity of variation this difficulty would be removed.
>
> It will be noted that it is impossible to suppose that the perfection of a variety, discontin-

[23] Merriam, *l. c.*, p. 245.

[24] Merriam, *l. c.*, p. 247.

[25] Cf. the map, p. 256.

[26] 'Species and Varieties,' pp. 741, 744, 749, 751, 800, 801, 802, 805, 825.

[27] Merriam, *l. c.*, p. 256.

[28] Ignoring again the continuous sporting of 'ever-sporting varieties,' and the distinctions between different kinds of sports.

[29] In the northern states white people are more common than black. Therefore, all the people in the northern states have originated from the whites. Q. E. D.

[30] Merriam, *l. c.*, p. 256.

[31] 'Materials for the Study of Variation,' p. 62.

uously and suddenly occurring, is the result of selection. * * * This consideration of course touches only the part that selection may have played in the first building up of the type and does not affect the view that the perpetuation of the type, once constituted, may have been achieved by selection.[32]

To say the least, the declaration that 'so far as known,' the theory of the origin of species by mutation is 'not applicable in the case of animals,' seems a rather arbitrary statement in the face of the mass of contrary evidence that exists in recent literature.[33]

Considering the volume of evidence in zoology[34] that 'distinct and perfect varieties may come into existence discontinuously,' the question forces itself, 'may not the discontinuity of species have had a similar origin?' At least the strong "presumption is created that the discontinuity of which species is an expression has its origin not in the environment," which is continuous, "nor in any phenomenon of adaptation, but in the intrinsic nature of organisms themselves, manifested in original discontinuity of variation."[35]

By a strange chance the article by Dr. Ortmann, in SCIENCE for May 11, 1906, immediately precedes one entitled 'Misrepresentations of Nature in Popular Magazines.' The history of science furnishes all too many instances of misinterpretations of *scientific theory* in *scientific* magazines. We do not recall an instance in which Darwinism (Darwin's Darwinism) has been more twisted out of shape than has the mutation theory in this latest exposure of its 'fallacy.' It would be interesting to know if the mutation theory itself would meet with such an 'emphatic' and wholesale 'condemnation' as does this misinterpretation of it.

We are told that "de Vries claims that the process of mutation forms new species, and that individual mutations (mutants) *are* species." The *title* of the English volume is 'Species *and Varieties*, their Origin by Mutation,' and one has to read only as far as the ninth page to learn that the author intends "to give a review of the facts obtained from plants which go to prove the assertion that species and varieties *have* originated by mutation, and are at present not *known* to originate in any other way," and on page 16 "Retrograde varieties and elementary [note the adjective] species may *both* be seen to be produced by sudden mutations."

And why beat about the bush and say that de Vries '*tries to* show that mutations breed true'? Why not frankly acknowledge the fact, so magnificently established by twenty odd years of painstaking experiment, and verified by other workers elsewhere, that the variations classed by de Vries as mutants do breed true?

If it is really true that de Vries 'does not know what constitutes a species,' then, indeed, do we find our faith in his work thereby increased. Who indeed, except the makers of dictionaries, does 'know what constitutes a species'? The author of the mutation theory does know, however, that 'genera and species are, at the present time, for a large part artificial, or stated more correctly, conventional groups,' and that, 'every systematist is free to delimit them in a wider or in a narrower sense, according to his judgment.'

Is it possible that one who can write, "What distinguishes species[36] from varieties is the fact that the species is not connected by intermediate or transitional forms with the closely allied species," has ever come into contact with a group like the hawthorns (*Cratægus*), or *Aster*, or the violets in botany, or the earwig (*Forficula*) in zoology? Whether this distinction 'is the one made use of exclusively (if possible)[37] by systematists, botanists as well as zoologists,' is, we believe, quite open to question. It was a consideration of the earwigs (*Forficula*) and crab (*Carcinus*) that led Vernon to state: "It is obvious, indeed, that between two absolutely dis-

[32] Bateson, *l. c.*, p. 69.

[33] *E. g.*, Bateson, *l. c.*, Vernon, 'Variation in Animals and Plants.'

[34] Space forbids citation of specific illustrations.

[35] Bateson, *l. c.*, p. 567.

[37] The meaning of the parenthesis is not clear.

[36] Meaning the systematic groups of specific value.

tinct varieties or species, and between pure monomorphic forms, all intermediate stages may exist."[38] Even Darwin considered that species, in the early stages of their history, were connected 'by intermediate gradations.'[39] Bicknell[40] has well said, 'The current ruling, more especially in vertebrate zoology, which, in view of * * * intermediates reduces to subspecies widely diverse organic forms, may well be suspected of being artificial and of attaching a fictitious importance to the mere evidences of origin which chance, perhaps, has allowed to remain unobliterated.'

Farlow concludes his paper on species by a sage remark: "I think we shall agree that in discussing the work of botanists in other departments than our own, it would not be wise to exact a rigid conformity to our individual conceptions of species, etc." It is another critic of mutation who tells us[41] that 'at the peripheries or borders' of faunal areas 'intergrades' between species[42] occur.

One who says that "de Vries has failed entirely to take notice of this fundamental [!] principle," surely can not have read that "new species and new varieties are seen to be quite free from their ancestors, and not linked to them by intermediates,"[43] and that the links between *elementary* species "often *apparently* overlap and can only in rare cases be determined on the sole ground of field observations," but that 'pedigree-culture is the method required,' etc.[44] Has the critic read: "Transitions are wholly wanting, although fallaciously apparent in some instances," as they appeared to be to Merriam,[45] "owing to the wide range of fluctuating variability of the forms concerned, or the occurrence of hybrids and subvarieties."[46]

If it is stated that de Vries "has failed * * * to show that his elementary species and his mutations" (*other* mutations, we suppose is meant) "are not connected by intermediate forms with each other," then we can not believe the writer has read the lucid definition: "any form which remains constant *and distinct from its allies* in the garden is to be considered as an elementary species,"[47] and that 'their limits often *apparently* overlap,' but may be determined by the method of pedigree-culture, though seldom 'on the sole ground of field observations.' The experimental facts were the source of the definition.

'Such intermediate forms are (indeed) recorded by de Vries himself,' together with his clear explanation of their significance. Bateson wrote over ten years ago: "We are concerned not with the question whether or no all intermediate gradations are possible or have ever existed, but with the wholly different question whether or no the normal form has passed through all these intermediate conditions."[48] One of the greatest values of de Vries's work lies in the fact that he was present when the transition took place, and gives, not a theory at all, but the record of a fact observed again and again.

If one is unable to see how de Vries can maintain that the mutations have bred true, as is stated on page 539 of 'Species and Varieties,' he has only to repeat the experiment himself to be convinced; and if it is implied, as it clearly is,[49] that *all* the mutants "were throwing off, in each generation, additional mutants," then the facts as recorded by de Vries are ignored. That mutation, however, may be a constant character,[50] just as truly as the shape of a leaf that constantly varies

[38] Vernon, *l. c.*, p. 41.

[39] Darwin, *l. c.*, p. 426.

[40] Torreya, 6: 94, 1906. Cf. also SCIENCE, February 16, 1906, p. 257, where Cook reduces three species and three varieties of *Utethesia* to one species *because of the intergradations.*

[41] Merriam, *l. c.*, p. 256.

[42] Or should we say 'subspecies'?

[43] 'Species and Varieties,' p. 18. 'Elementary' species plainly from the context.

[44] 'Species and Varieties,' p. 18.

[45] Merriam, *l. c.*, p. 243.

[46] 'Species and Varieties,' p. 249.

[47] 'Species and Varieties,' p. 12.

[48] 'Materials for the Study of Variation,' p. 42.

[49] Ortmann, p. 747.

[50] During the mutation period of the species, if the theory of mutation periods ('Species and Varieties,' Lecture XIV.) shall finally become established, and excepting, of course, the 'stray mutations' ('Species and Varieties,' pp. 704–706).

(fluctuating variation), is clearly brought out by the author of the mutation theory. And if, in speaking of 'de Vries's contention that mutations are species,' one has in mind the units of the systematist, then it must be insisted that such is not de Vries's contention at all, nor can such a statement be wrested from the book, 'Species and Varieties,' if the context is always considered.

"However, if de Vries had claimed that species might be made out of mutations, nothing could be objected to this view."[51] Exactly! Then why the dissenting critique? Read, on page 13 of 'Species and Varieties,' the following:

> Linnæus himself knew, that in some cases all subdivisions of a species are of equal rank, together constituting the group called species. No one of them outranks the others; it is not a species with varieties, but a group consisting only of varieties. A closer inquiry into the cases treated in this manner by the great master of systematic science, shows that here his varieties were exactly what we now call elementary species.

And this on page 558: "*The first law* [*of mutation*] *is, that new elementary species appear suddenly, without intermediate steps.*"

Finally (page 459), "Hence we have distinguished between elementary species and varieties proper. THE FIRST ARE COMBINED INTO SPECIES" (of the systematist), etc. Could a clearer statement be conceived?

There is, indeed, a sense in which this is 'no new idea,'[52] but the recognition of it, and of its bearing upon theories of descent seems to be not only new, but difficult at the present day of being clearly recognized and understood. De Vries does not claim the credit of originating the idea. He proposes to found his theory, in part, upon "a critical survey of the facts of agricultural and horticultural breeding, as they have accumulated since the time of Darwin."[53]

Says our critic: "The breeding of domestic races has always (!!) been regarded as a process analogous to the one in nature by which new species are produced." When, we may be allowed to ask, previous to the appearance of the 'Origin,' had such an idea been seriously or at all generally held. If we have not access to the original, we may even learn from 'Species and Varieties,' if we read carefully, that Linnæus looked upon species as the result of special creation."[54]

The failure to take some of his statements at their face value, or at least to distinguish clearly when he is recording a fact and when he is elaborating a theory, is a peculiar feature of the criticisms of de Vries, as in the present critique, where it is stated that he 'finally obtained *more or less* pure strains.'[55] An equally peculiar and persistent feature of these criticisms is the insistence with which the author's carefully defined terms, representing equally careful and long-needed distinctions, are entirely ignored, thus giving to his statements a wholly different color than they possess in the original. *E. g.*, "Before he [de Vries] began this process of selecting and segregating, the mutations were by no means species, but only varieties." Where, from cover to cover, of 'Species and Varieties,' is any other claim made? The point is that the author very carefully states *what kind* of varieties they are, and applies to this kind the term 'elementary-species.'

Thus again, in the next sentence, "de Vries further maintains that it is the mutations and not the variations, that give rise to new species, and he thinks [*sic*] that there is a fundamental difference between them." A clear conception of the mutation theory would have resulted in some such changes as the following in that sentence: "de Vries further *gives experimental evidence* that it is the mutations, *and not the other types of variation,* that give rise to new species, and *between these kinds of variation* there is a fundamental difference." The critic might have added here, also, the statement, 'but this no new idea,' and might have quoted from Bateson:

> The existence of discontinuity in variation is, therefore, a final proof that the accepted hypothesis is inadequate. If the evidence went no further than this the result would be of use, though

[51] Ortmann, p. 747.
[52] Ortmann, *l. c.*, p. 747.
[53] 'Species and Varieties,' p. 9.
[54] 'Species and Varieties,' p. 34.
[55] Ortmann, *l. c.*, p. 747. Italics mine.

its use would be rather to destroy than to build up. But besides this negative result there is a positive result too, and the same discontinuity which in the old structure had no place may be made the framework round which a new structure may be built.[56]

And the following cautious statement from Darwin: 'We are led to conclude that species have *generally* originated by the natural selection, not of abrupt modifications, but of extremely slight differences.'[57] Their origination by the natural selection of the 'abrupt modifications' is rejected, among other reasons, because "we have *no evidence* of the appearance, or at least of the continued procreation, under nature, of abrupt modifications of structure."[58] Since the work of de Vries the last statement no longer holds true.

The critic "is unable to see where he [de Vries] draws the line between variations * * * and mutations." The following quotation seems written in anticipation of that statement: "*The relation between mutability and fluctuating variability* has always been one of the chief difficulties of the followers of Darwin.[59] The majority assume that species arise by the slow accumulation of slight fluctuating deviations, and the mutations are only to be considered as extreme fluctuations, obtained, in the main, by a continuous selection of small differences in a constant direction."

"My cultures show that quite the opposite is the fact. * * * Oscillating changes have nothing in common with the mutations."[60] "Mutations are going on in all directions, producing, if they are progressive" (they are not all alike), "something quite new every time. Fluctuations are limited to increase and decrease of what is already available."[61]

One point is stated with terse accuracy: de Vries has not only not 'solved the old sophistic problem of how much must be added to a small thing to make it a large one,' but has clearly demonstrated, in his lecture on 'The Origin of the Peloric Toad-Flax' that, so far as the origin of species is concerned, the problem is incapable of solution.

The discoverer of 'the fallacy of the mutation theory' tells us that mutations have been observed chiefly among domestic forms. If this were true, the suggestion is, not to reject mutation on this account, but to initiate extensive experiments among a wide systematic range of wild plants (and of animals also), and see if what now appears to be the case is in reality a general truth, or only an expression of limited experience.

We are told[62] that if de Vries should 'claim that species could be made out of mutations, he would be right.' Well and good. The quotation[63] has already been given. Elementary species 'are combined into species * * *.'

If the rarity of mutations[64] seems to be a stumbling-block toward accepting them as the material on which natural selection may operate, it will be wholesome to recall that the formation of new specific groups (species of the systematists) is far more rare in nature than in the writings of systematic botanists and zoologists.

Mutation has, indeed, 'always'[65] been regarded as a special form of variation, and so it is in 'Species and Varieties,' notably in lecture I., and throughout the entire book. But if 'Consequently, nothing is left of de Vries's mutation theory but the bare facts represented by his experiments,' science has been munificently enriched thereby, to say nothing of the new method of research in the study of evolution, entirely ignored by the above statement, and the contribution for which science is de Vries's greatest debtor.

"To my mind," says de Vries, "the real value of the discovery of the mutability of the evening-primrose lies in its usefulness as a guide for further work."

I may repeat, in closing, what was stated in

[56] Bateson, *l. c.*, p. 568, 1894.

[57] 'Animals and Plants Under Domestication,' p. 495. Italics mine.

[58] Darwin, *l. c.*, p. 495.

[59] 'Species and Varieties,' p. 7. Even Wallace had this difficulty. *Ibid.*

[60] 'Species and Varieties,' pp. 568–569. Cf. pp. 715, 718 and 459.

[61] 'Species and Varieties,' p. 719.

[62] Ortmann, *l. c.*, p. 747.

[63] From 'Species and Varieties,' p. 459.

[64] Ortmann, *l. c.*, p. 747.

[65] That is, since the process has been recognized and described.

the beginning. I have not attempted to defend the mutation theory of de Vries, but only to emphasize the fact that, before we criticize it, or lend to it either our dissent or assent, we must first understand it. The ink that was spilled in discussing misinterpretations of Darwinism far exceeds that poured out in recording constructive studies in evolution. Let us not make the same mistake and waste of energy in the present case.

The mutation 'theory' is still largely a working hypothesis. It is founded almost entirely upon experiment, and can be verified only by the same means. The beauty of it is that it is already reduced to a question of fact. For mere opinion and inference, and *a priori* impressions and prejudice, and inductions from field studies and comparative morphology there is absolutely no place. If one doubts the facts, let him repeat the experiments of de Vries and MacDougal and others. If he doubts that they represent a general truth, a fundamental principle in biology, then let him await the fullness of time, for it is by repeated experiment, among a wide range of groups, and *by experiment only*, that the general application must stand or fall.

And I bespeak also a candid acceptance of the facts, after they are clearly distinguished from the inferences. The latter are open to debate, but not so the former. And when a careful worker says that he obtained a given form that breeds absolutely true, and which, for reasons fully explained, he calls an 'elementary species,' by means of a certain definite and clearly explained kind of variation which he defines and names 'mutation,' let us not refer to him as 'claiming to' have done so, or to the mutant as 'seeming to' breed true.

Pregnant with significance as the mutation theory is for the systematic botanist and zoologist, its truth can never be established nor disproved by the methods of taxonomy. Comparative studies may offer worlds of evidence and multitudes of problems to test the hypothesis, but experimentation is the only possible means for the final solution.

How do species originate? A mass of facts suggests that the method is by the natural selection of fluctuating variations, combined with geographical isolation, influence of environment, and other factors. But, after all has been written, the undeniable fact remains that no one has yet ever actually observed the origin of a single species in this way.

On the other hand, the fact is just as undeniable that a definite and clearly defined type of variation, called an 'elementary species,' has been actually observed, not once, but often and by many, to arise by a process, equally well defined and definite, and known as 'mutation.' Mutations do furnish material for the operation of natural selection and all other influences that tend to establish a unit group known in taxonomy as a 'species.'

The case seems perfectly plain that the burden of proof rests with the adherents of fluctuation.

C. STUART GAGER.
NEW YORK BOTANICAL GARDEN.

SPECIAL ARTICLES.

A NEW FOSSIL SEAL FROM THE MARINE MIOCENE OF THE OREGON COAST REGION.

IN a bulletin recently issued by the Oregon State University, Professor Thomas Condon has given a description of an unusually interesting fossil pinniped, which was obtained by him from the Marine Miocene of the Oregon coast. It is, indeed, a happy coincidence that this nestor of Oregon geology should celebrate his eighty-fourth birthday by so interesting and important a discovery. This does not quite equal the record of the great chemist, Chevreul, before the French Academy, it is true, but it is one sufficiently rare in paleontology.

Professor Condon has kindly permitted me to make a careful study of this unique specimen, and I do not hesitate to pronounce it easily the most important find that has yet been made in this group. As far as I am aware the specimen represents an entirely new and hitherto unknown genus, intermediate in many respects between the sea lions and seals, with perhaps the most pronounced affinities with the latter, and at the same time exhibit-

ing a number of primitive or ancestral characters not found in the skeleton of any modern pinniped. He has appropriately named the new genus *Desmatophoca oregonensis*.

The age of the horizon in which this fossil was found is determined by Professor Condon to be Miocene from the association of a large number of characteristic genera of marine shells. Whether it represents the entire series of fresh-water deposits included in the Oligocene and Miocene of the interior continental region or whether it is equivalent to only a part of them can not now be determined. I think it may be safely assumed, however, to be the equivalent of the entire series, although its vertical thickness is considerably less than the contemporary fresh-water beds. The position of the fossil within the deposit was not accurately recorded but there is reason to believe that it comes from a layer which would suggest a Middle Oligocene age if referred to the interior series.

The point of greatest interest and importance in connection with the fossil under consideration is to be found in the light which it throws upon the broader question of the origin of this highly modified and almost exclusively aquatic branch of the carnivorous mammals. Hitherto an understanding of the more exact genetic connections between the Pinnipedia and the land Carnivora has proven a very obscure and perplexing problem to the morphologist, and as a result various opinions have been expressed concerning its solution. There seems, however, to have been entire unanimity of opinion in the hypothesis that the pinnipeds are descendants of some member of the land Carnivora and that their extreme modification has been due to the assumption of an aquatic habit with the attendant changes in structure necessitated by this environment.

Huxley held that the seals (using this term in its broader sense) have been derived from the bears and he attempted to point out similarities of structure between the two groups. In this view Mivart entirely concurred. The chief objections to this hypothesis are the following: The bears have now been pretty thoroughly proven by Schlosser to be descendants of the canoid or dog phylum, through *Amphicyon*, dating no further back than the later Oligocene or earlier Miocene. In the fossil seal before us we have a very distinct pinniped, at least equivalent in age to *Amphicyon* exhibiting no approach whatever to the dog or bear groups. This hypothesis may, therefore, be dismissed as wholly untenable.

The next expression of opinion on this subject was made by myself in describing the osteology of *Patriofelis*. In this publication I pointed out the large assemblage of creodont characters to be found in the skeletons of modern pinnipeds and from a careful analysis of these features in comparison with those of certain members of the Creodonta I was led to the conclusion that the seals are the direct descendants of the creodonts. I further held that the particular family among the latter which exhibits the closest approximation and furnishes the most likely ancestral beginnings thus far known is the Oxyænidæ.

Osborn, followed by Matthew and Scott, has dissented from this view and failed to discover any relationship between the creodonts and pinnipeds. Osborn's argument against the hypothesis was based wholly upon an effort to disprove the supposed aquatic or semiaquatic habits of certain of the Oxyænidæ, which he concluded were preeminently terrestrial or arboreal. Matthew's argument consists of a simple denial of the alleged relationship of the two groups. It now remains to reexamine the subject in the light of this newly acquired evidence and test the hypothesis of a creodont ancestry by this oldest and most primitive pinniped thus far discovered.

The specimen consists of a fairly well preserved skull in which the larger part of the dentition, especially that of the upper jaw, is present. The most important feature of the teeth is the evidence they afford of their derivation from a former more complex or tubercular condition. This is especially marked not only by the two- and three-rooted

manner of their implantation, but by the more complex structure of the crown of the fourth superior premolar as compared with that of the modern seals. This tooth may be said to be implanted by three roots, of which the one supporting the inner cusp is not entirely distinct at the base, at least. The crown exhibits the remains of three distinct cusps corresponding in every way to those of the less specialized sectorial of many of the creodonts. It is somewhat simpler in structure than the corresponding tooth of *Patriofelis,* but its derivation from a tooth of that character is clearly evident. The single molar, both above and below, is missing from the specimen, so that the organization of the crowns can not be determined. They were each implanted by two distinct roots. The dental formula is I. 3/2, C. 1/1, PM. 4/4, M. 1/1.

One character which is most unusual for a seal is the deep heavy horizontal ramus of the lower jaw, together with a relatively powerful symphysis reminding one at once of the corresponding parts of *Oxyæna* and *Patriofelis* among the creodonts. The impression which one immediately receives upon examination of the specimen is that of a short heavy-jawed animal. I mention this character in particular for the reason that the heavy jaw and powerful symphysis of *Patriofelis* was made the basis of an especial objection, on the part of Osborn, to any possibility of affinity between the two. The jaws of the modern seals are relatively weak and slender.

In the cranium the more important characters to be noted are the rudimental postorbital processes, the marked constriction of the postorbital region, the relatively heavy zygomatic arches and the peculiarly distinctive creodont organization of the otic region of the skull. The mastoid is of moderate proportions and the widely separated paroccipital process is unusually large and massive and projects outwards and backwards. This arrangement and unusual size of the paroccipital is one of the most distinctive characters of the creodont skull and is one not found in any of the modern Carnivora. I may add likewise that its condition in this ancient seal is almost identical with that seen in *Patriofelis* and *Oxyæna.*

The base of the skull had not been sufficiently freed from the inclosing matrix to permit of an accurate determination of the characters of this region, but I think the presence of a small rugged uninflated bulla and an alisphenoid canal may be assumed. The occipital crest is low and inconspicuous and does not overhang the occiput as in the creodonts. The brain case is large and roomy and the brain was considerably convoluted. In fact the brain case and occipital region of the skull resemble that of the modern *Phoca* closely.

From a consideration of the foregoing characters of this important specimen the following conclusions appear to be firmly and conclusively established: (1) the seals have been derived from ancestors in which the molars were tritubercular; (2) in which there were but two pairs of incisors in the lower jaw; (3) in which the true molars were early reduced; (4) in which the lower jaw was stout and heavy with a strong, heavy symphysis; (5) in which the mastoid region of the skull was identical with that of certain of the creodonts; (6) in which the postorbital process was rudimentary or wanting.

Less firmly established characters of the ancestors of the seals may be enumerated as follows: (1) Double tongue and groove articulations of the lumbar vertebræ; (2) early development of aquatic habits with consequent modification of limbs.

If next we direct our attention to the application of these characters to the known fossil Carnivora which preceded them in time, we find that it is only among the creodonts that these characters are to be met with. It, therefore, follows that a creodont ancestry is the only possible or logical solution of the problem. As to the particular family of creodonts from which the seals have been derived the evidence is by no means so clear nor conclusive. As far as now known the choice seems to be restricted to either the Hyænodontidæ or the Oxyænidæ. Of these the latter are

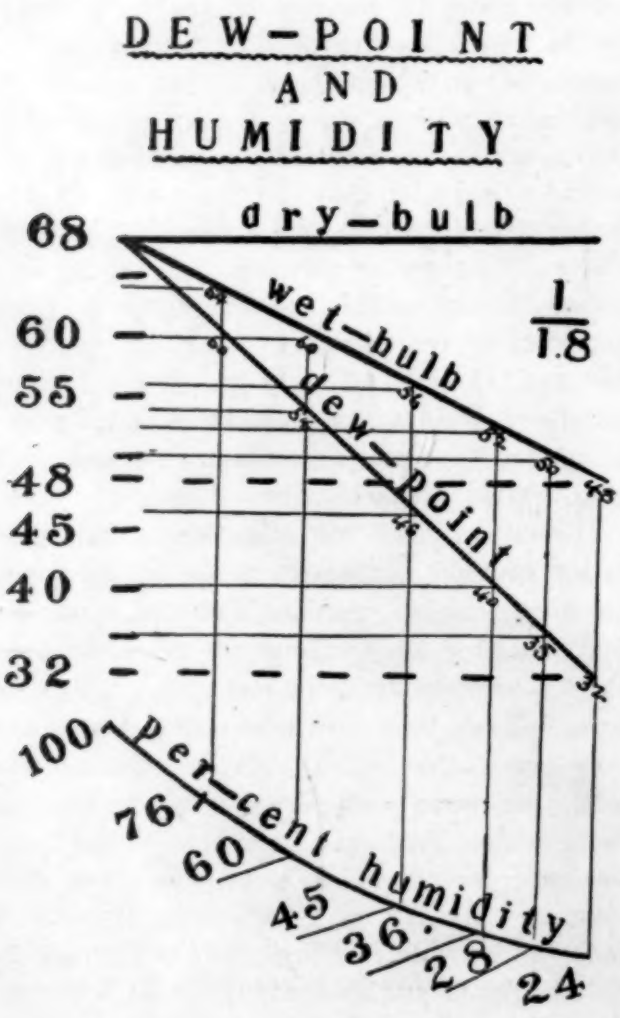
much the more likely, for the reason that all the conditions are more completely satisfied. No hyænodont is known in which the molars are reduced, whereas among the Oxyænidæ molar reduction is one of the most pronounced characters. In fact all the primitive characters are identical with those of this group. I take this occasion, therefore, to reaffirm the opinion I expressed on this subject some twelve years ago and I do so without modification or emendation.

J. L. Wortman.

McMinnville, Oregon,
June 6, 1906.

DEW-POINT AND HUMIDITY CHART.

The chart shows dew-point and relative humidity in a room whose temperature is kept at about 68°. These are readily calculated from the readings of a wet-bulb thermometer kept in the room. So long as the temperature is kept near 68°—say between 66° and 70°—the difference between the readings of the dry-bulb thermometer and the dew-point is always about 1.8 times the difference between the readings of the dry- and wet-bulb thermometers. The percentage of humidity, which corresponds approximately to these readings, is shown in the curved line below.



John F. Woodhull.

Teachers College,
Columbia University.

QUOTATIONS.

THE MOST IMPORTANT WORK IN THE WORLD.

The bringing of scientific agriculture into general practise is, we presume, the most important economic task that awaits us; and it is more than an economic task. In laboratories and on small experimental areas, methods have already been worked out which, if universally applied, would so increase the yield and the quality of our great crops, and consequently the profit of growing them, that the culture of the earth would become more profitable than commerce and manufactures. The ambitious young men have left the farms for the cities, from Abraham's day, if they had cities then, till our own, because they could make more money in trade and in similar pursuits; and the farmer, as a rule, has been the left-over man; and he will be so, till this economic situation is changed.

Great hopes were entertained a generation ago that the agricultural colleges would teach men scientific farming; and so they have; but most of the men who have thus been taught have themselves become teachers and have taught others who in turn have become teachers; and the man on the soil has, as a rule, not yet been reached with the new knowledge and with new methods.

Agricultural bulletins, too, have done good, but they have instructed those who least needed instruction; for the typical farmer does not learn farming by reading about it. Experiment stations have had a more direct influence and have caused better methods to be used in their neighborhoods.

But all these good agencies have yet failed to reach the mass of men who till the earth, the thousands and hundreds of thousands of

farmers who plow and sow and reap as their fathers did and who are suspicious of innovations, of book-farming, and of new ideas in general.

It remained for the National Agricultural Department, by a stroke of that common sense which we call genius, to begin the work of 'demonstration' on the farms of farmers who themselves work them. A report has been published by the Bureau of Plant Industry which explains the 'farmers' cooperative demonstration work' done in Texas and Louisiana under the direction of Dr. S. A. Knapp; and that is a pamphlet which seems likely to show a new hope for mankind.

The method of instructing farmers is simplicity itself. A demonstrator goes to a farmer and persuades him to do two or three such simple new things as to prepare his land in the fall or winter, to plow it deep, to practise intensive farming—that is, to cultivate it better—and to select his seed. This pamphlet is made up of reports from these 'demonstrators.' Wherever one farmer has once done these things on a small area under the direction of a demonstrator, the results have caused a change in the general agricultural practise of the neighborhood. The whole problem is to do such work in every neighborhood. These reports contain such remarks as these:

> Six years ago an average yield of 30 per cent. of lint cotton was considered very good. Now we often have cotton that yields as high as 38 per cent. of lint. That alone in the cotton crop of the South means a profit of about $30,000,000. (From Palestine, Texas.)
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> The seed we gave out last season produced from a third to three times as much as the old varieties. At Grosbeck, where 7,000 acres of cotton will be planted this year, a good season will produce 1,000 more bales than the same acreage would have yielded planted in the old way. (From Houston, Texas.)
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> In 1904 I had to be very careful how I approached a farmer. He would say he cared nothing about our book farming. Now they insist on my going to see them. There is 50 per cent. improvement in our agriculture as compared with a few years ago. (From Shreveport, La.)

The area over which this kind of instruction is carried on has this year been greatly extended. If this be not education that tells, then what is? One philosophical observer of this movement has called it 'the most important work in the world.'—*The World's Work.*

OBSERVATORIES AND ASTRONOMERS OF THE WORLD.

THE Committee of Bibliography and of Astronomical Sciences of the Royal Observatory of Belgium has undertaken to publish a list of the observatories and astronomers of the world. A request for information, in the form of a list of questions, with a model reply relating to the astronomical service at the Uccle Observatory, Belgium, has been addressed to all the directors of observatories. In addition the list will include such astronomers (university professors, amateurs, etc.) who are not attached to any observatory, but are, nevertheless, actively engaged in astronomical research. The information already sent will enable the committee to draw up not only a list of observatories, with their geographical coordinates and the members of the staff, but also a table showing the astronomical activity of the world, thanks to the facts given as to the instruments at the disposal of each institution, the pieces of research undertaken, and the papers published. The directors of those observatories who have not received the question-form, or who have not yet forwarded a reply, as well as unattached astronomers, are requested to send the information desired, as soon as possible, addressed to the chairman of the committee, Professor P. Stroobant, astronomer at the Royal Observatory of Belgium, Uccle, Belgium.

ELIZABETH THOMPSON SCIENCE FUND.

THE 31st meeting of the board of trustees was held at the Harvard Medical School, Boston, Mass., on June 25. The following officers were elected:

President—Henry P. Bowditch.
Treasurer—Charles S. Rackemann.
Secretary—Charles S. Minot.

Professor Bowditch offered his resignation as trustee, since he had now withdrawn from active participation in scientific work. The

trustees expressed their reluctance to accept this resignation, because of the long and valued service of Professor Bowditch, yet they desired to relieve him of burdensome responsibility. It was voted to accept the resignation to take effect when through correspondence a successor has been elected.

The secretary reported that on December 12, 1905, a grant of $1,000 had been awarded to Professor Angelo Mosso for the establishment of an American table at the International Mountain Laboratory, founded by Professor Mosso under the auspices of the Italian government, on the Col d'Olen of Monte Rosa. It was specified that appointments to this table should be made by the trustees of the Elizabeth Thompson Science Fund. Of the sum granted, $500 was received as a special contribution for this purpose from the Bache Fund of the National Academy of Sciences in Washington.

It was voted to close the records of the following grants, the work having been completed and publications made: No. 60, F. Kruger; No. 110, H. S. Grindley; No. 114, W. Rosenthal; and to close upon receipt of publication grant 113, made to S. P. Fergusson.

Reports of progress from the following holders of grants were received:

No. 94. A. M. Reese.
" 96. H. E. Crampton.
" 98. J. Weinzirl.
" 101. T. A. Jaggar, Jr.
" 103. E. Anding.
" 105. H. Kronecker.
" 106. W. Valentiner.
" 107. M. Travers.
" 108. B. L. Seawell.
" 109. A. Nicolas.
" 111. R. Hürthle.
" 115. H. S. Carhart.
" 116. W. Bateson.
" 117. E. Salkowski.
" 118. Th. Boveri.
" 119. J. P. McMurrich.
" 120. E. H. Archibald.
" 121. A. Debierne.
" 122. J. J. Frič and Fr. Nušl.
" 123. E. C. Jeffrey.

No report was received from Professor P. Bachmetjew concerning grant 124.

The treasurer announced that $1,300 was available for new grants. It was necessary to decline several applications which the trustees would have favored if the resources had permitted. The following new grants were made:

No. 126. $250 to Professor L. Cuénot, Nancy, France, for researches on heredity in mice.

No. 127. $250 to Professor E. Wiedemann, Erlangen, Germany, to study the behavior of gases at high temperatures for astrophysical purposes, and to determine their illuminating value.

No. 128. $50 to Dr. L. J. Henderson, Boston, Mass., to study the compressibility of muscle.

No. 129. $50 to Dr. G. W. Hall, Boston, Mass., to investigate the oxidation of glucose and allied substances in the organism.

No. 130. $125 to B. G. Smith, Esq., Ann Arbor, Mich., for the study of the embryology of *Cryptobranchus*.

No. 131. $100 to Dr. F. W. Thyng, Boston, Mass., to complete the study of a human embryo of 13.6 mm.

No. 132. $200 to Professor W. G. Cady, Middletown, Conn., to investigate the electric arc between metallic electrodes.

Final action regarding the application of Dr. H. H. Field, concerning the card catalogue for physiology, was postponed.

Charles S. Minot,
Secretary.

SCIENTIFIC NOTES AND NEWS.

Professor van't Hoff, of Berlin, was elected an honorary member of the German Chemical Society at the recent meeting at Nuremberg.

Lieut.-Colonel D. Prain, F.R.S., director of the Royal Gardens, Kew, and late director of the Botanical Survey of India, has been appointed a Companion of the Order of the Indian Empire.

M. Gernez, professor of general chemistry in the School of Arts and Manufactures, Paris, has been elected a member of the Academy of Sciences, to succeed the late M. Curie.

Jesse B. Mowry, B.S., has been appointed, by Governor Utter, commissioner of forestry for Rhode Island.

DR. E. ANDING, professor in the University of Munich, has been appointed director of the Ducal Observatory in Gotha.

M. JANSSEN, director of the Astrophysical Observatory of Meudon, Paris, will represent the Paris Academy of Sciences on the occasion of the celebration of the fourth centenary of the University of Aberdeen, on September 25.

PROFESSOR NOAH K. DAVIS, who is now seventy-six years of age, has retired from the active duties of the chair of moral philosophy in the University of Virginia.

AT an assembly of the faculties of University College, London, on July 4, Dr. Carey Foster, emeritus professor of physics and former principal of the college, was presented with his portrait, painted by Mr. Augustus John.

THE former pupils of Dr. Hallaran Bennett, professor of surgery in the School of Physic, Trinity College, Durham, and for forty-five years a member of the teaching staff, have established a medal to be awarded in his honor, and will place a bronze life-size relief portrait in the school and in Sir Patrick Dun's Hospital.

AT a meeting of the officers and council of the Norwegian Geographical Society, in Christiania, on May 19, the gold medal of the society was awarded to Dr. Carl Lumholtz for his scientific explorations. The medal will be presented to Dr. Lumholtz in September, on his arrival in Christiania.

THE Longstaff medal of the London Chemical Society has been awarded to Professor W. N. Hartley, F.R.S., in recognition of his spectrochemical investigations.

SIR LAUDER BRUNTON, Dr. J. C. McVail, medical officer of health for the counties of Stirling and Dumbarton, Dr. A. Macfadyen, of the Lister Institute, and Dr. T. A. Starkey, professor of hygiene, McGill University, have been elected fellows of the Royal Sanitary Institute.

The Journal of the American Medical Association states that Professor C. F. Schatz, the eminent German obstetrician and professor of gynecology at Rostock, was accused of misappropriating several thousand dollars of hospital funds. He insisted on a thorough investigation, and the trouble was found to be the result of a clerical error in auditing the accounts of the hospital. He has resigned his connection with the university and will devote himself to private practise.

DR. C. S. PRINGLE, keeper of the herbarium of the University of Vermont, has started on his twenty-sixth annual journey to Mexico for botanical exploration. He expects to return to the university in the autumn and make a distribution of the plants collected during the past year.

AN expedition has been sent by the Peabody Museum of Archeology of Harvard University to northern New York to explore an ancient Iroquois site, in continuation of the work of the past two years in that state. Mr. M. R. Harrington, of Columbia University, who received his archeological training under Professor Putnam, will be in charge of the expedition. Mr. Irwin Hayden, a graduate student in the division of anthropology, will be Mr. Harrington's assistant. Mr. Ernest Volk will be engaged for a portion of this season to continue the investigations relating to the antiquity of man in the Delaware Valley, where a careful examination will be made of the glacial deposits near Trenton, N. J. For the past seventeen years Mr. Volk has been associated with Professor Putnam in carrying on this research.

Nature announces that it is proposed to establish some permanent memorial of the late Professor W. F. R. Weldon, not only of the man himself, but also of the movement with which his name is especially associated, the application, that is, of exact methods of statistical inquiry to the study of variation and kindred problems in zoology. It has been suggested that the memorial should consist of a portrait—medallion or bust—in the museum at Oxford, a cast of which might be placed in University College, London, and of a prize to be awarded periodically to the author of the most valuable biometric publication of recent date. The committee will arrange that sub-

scribers may eventually purchase a reproduction of the portrait. Contributions may be sent to Dr. G. C. Bourne, Savile House, Oxford; Dr. G. H. Fowler, 58 Bedford Gardens, W.; Professor Karl Pearson, F.R.S., University College, W.C.; Mr. Adam Sedgwick, F.R.S., Trinity College, Cambridge; or to the Weldon Memorial Account, at the Old Bank, Oxford.

A MONUMENT in memory of Professor Nocard was unveiled recently in the Veterinary School of Alfort by M. Ruan, minister of agriculture.

SEÑOR MANUEL GARCIA, the well-known teacher of music, and eminent for the invention of the laryngoscope, died in London, on June 30. It will be remembered that the hundredth birthday of Señor Garcia was celebrated on March 17, 1905.

THE death is announced of M. Rayet, director of the observatory and professor in the University of Bordeaux.

DR. PHILIP OSSIANNIKOW, professor of anatomy and physiology at the University of St. Petersburg, died on June 11, at the age of seventy-nine years.

A NATIONAL ACADEMY OF SCIENCES has been established in Japan. It will contain forty members, of whom fifteen are to be nominated by the government and twenty-five elected.

THERE will be an international conference on cancer research in connection with the opening of the Institute for Cancer Research at Heidelberg, on September 24.

THE offer of the New York Zoological Society to the government to provide a herd of buffaloes for the Wichita Forest Reserve, in southwestern Oklahoma, has been accepted. The society will supply a herd of between fifteen and twenty buffaloes, and the government will fence in a suitable range on the game reserve. For this purpose an appropriation of $15,000 was included in the agricultural bill.

UNIVERSITY AND EDUCATIONAL NEWS.

MR. CLARENCE H. MACKAY and Mrs. John W. Mackay have given $100,000 to the University of California, to endow the chair of electrical engineering. It will be known as the John W. Mackay, Jr., professorship, in memory of Mr. Mackay's brother, and will be filled by Professor C. L. Cory, head of the department of mechanical and electrical engineering.

THE newly-organized School of Forestry of Colorado College will begin work at the opening of the college term in September. The courses in engineering, chemistry, physics, geology and biology will be given at the college in Colorado Springs. The instruction in forestry proper will be given partly in Colorado Springs and partly in Manitou Park, a forested area of 13,000 acres, situated in the mountains at a distance of twenty miles from the city. This tract has been recently presented to the college for the use of the School of Forestry, and a number of cottages have been provided for the instructors and students.

DR. GEORGE E. FELLOWS, president of the University of Maine, has declined the offer of the presidency of the Pennsylvania State College.

DR. JAMES LOUDON has, on the ground of ill health, resigned the principalship of the University of Toronto.

DR. RAYMOND PEARL, instructor in zoology at the University of Michigan, has been appointed instructor in zoology at the University of Pennsylvania.

LOUIS M. TERMAN, Ph.D. (Clark, 1905), principal of the San Bernardino High School, California, has been appointed professor of pedagogy at the State Normal College at Los Angeles.

C. S. MYERS, M.A., M.D., has been appointed professor of psychology at King's College, London. At the same institution H. S. Allen, B.A., B.Sc., has been appointed senior lecturer in physics.

PROFESSOR ALBERT NARATH, of Utrecht, has been appointed professor of surgery in the University of Heidelberg, to succeed Professor Czerny, who has retired to take charge of the new Cancer Institute.